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ON INDUCTIVE INFERENCE

ALL probabilities are man-made. They result from limited vision. If we knew things as they are our statements regarding them would be assertions of truth or falsity—and it is only when we are dealing with matters whose certainty is questionable that we make statements regarding that which is probably true. To know the exact nature and extent of that which is questionable among all the information which is used in making inductions is a highly important matter. Yet we regard our statements about probability as definitely not-questionable. It is true that so and so is probable, first, because there is a certain bit of truth available, and, secondly, because there is in addition the truth that something is questionable. Both of these matters must be explained in justice to any system of reasoning which has probability for its end, and the meaning of *probable* must be exactly defined.

The word, *probable,* as W. Stanley Jevons has pointed out, is essentially the same as the word, *provable.* Though for modern usage the provable is that which can be demonstrated, it has not always been so nor is it now so in all languages. When Christopher Marlowe's Passionate Shepherd addressed his lady—

> "Come live with me and be my love,
> And we will all the pleasures prove
> That grove or valley, hill or field,
> Or wood and steepy mountain yield,"

he evidently meant to suggest that they should test, examine, and find good, that is, verify, the various pleasures to which he referred. And in German the word for test is *Probe.* But if the probable is merely the verifiable, or that which may be tested *and found,* then indeed it is impossible to understand in what sense a belief that works is, as we commonly say it is, probable. For the belief that works is a belief relative to a situation which is not accessible. Could we have access to the situation, we should not have a probably true workable belief, but would be confronted with fact. Between hypotheses, which are verified in their consequences, and the laws of science, which are repeatedly verified by particular fact, there is a distinct logical difference; and to call them testable in the same sense is to speak loosely. Moreover, if the probable were merely the testable there would be no question of inductive logic—not that that would be a calamity—but there would nevertheless remain the question, "What do we mean when we say that a belief which has been repeatedly tested is probably true?" A few things considered, *testable* and *probable* are not synonymous.

For the classical logic inductive reasoning was reasoning from the particular to the general. The so-called "inductive or Baconian method" in science, the introduction of which has been ascribed, probably falsely, sometimes to Roger and sometimes to Francis Bacon, may be formulated thus:

(1) *A* explains *B*, and *B* is observed.
(2) *A* explains also *C, D, E, etc.*, and these are sought out and found.
(3) *A* is probably true.

To be sure this formulation may be taken as a definition of probable; but it never is, and need not be, so taken—and consequently the question of the meaning of the word and of the justice of the reasoning still remains.

From such reasoning to the theory of probability and the various calculations that are based upon it seems a long step: yet both are grounded in the same logical formation.

The Laplace theory of probability is based upon the common observation that judgments about probability must somewhere involve an element of uncertainty. But Laplace made the serious error of basing the judgment wholly on ignorance in those cases in which no information was to be had. Thus, before a coin is flipped the probability that it will "come heads" is equal to the probability that it will "come tails"—because we have no information at hand which leads us to believe that it is more likely to behave one way than the other. The absurdity inherent in such reasoning has been brought out by Edgeworth, who asked if the probability is fifty-fifty that there are black beetles on the moon, merely because there is no information about it either way. It appears then that probability somehow involves the right balance of ignorance with knowledge. But what is such a right balance?

Suppose the follower of the Laplace theory flips his coin, say a hundred times, and that it falls heads fifty-four times out of the hundred, the extent of his ignorance is that he does not know how it will fall the next time he flips it; the extent of his knowledge, that it has fallen heads fifty-four times out of a hundred. Basing his judgment now on the knowledge that he has available, he judges that the probability that it will fall heads the next time he flips it is fifty-four in a hundred. Here is a certain balance of ignorance and knowledge. But we may still seek for the warrant for his judgment—does he invoke the principle that the next hundred times the coin is flipped it will behave as it did during the hundred that he has already examined? or does he only have a feeling, a "hunch" as the slang has it, that because it has behaved in a certain way in one set of experiments it is likely to behave similarly in another set? and is the word *probable* only a cover for his hunch?

In the still simpler case, where any two properties or phenomena, *A* and *B,* are found associated or in any other relation, and are found so repeatedly while no counter-instances are observed, then by establishing a certain balance of knowledge and ignorance, one might reason, as Mr. Bertrand Russell does reason, that "The greater the number of cases in which a thing of the sort *A* has been found associated with a thing of the sort *B,* the more probable it is (if no cases of failure of association are known) that *A* is always associated with *B.*"[1] Mr. Russell thinks that the mere enumeration of instances is sufficient to establish a probability—an opinion against which there are a number of cogent arguments. As this question, however, is not in the direct path to the question of the nature of probability, and as a definite answer to it is given by an answer to that question (as will be shown), it need not be further agitated at the moment. Mr. Russell also believes that "a sufficient number of cases of association of *A* with *B* will make it nearly certain that *A* is always associated with *B,* and will make this general law approach certainty without limit."[2] This view seems to be nothing but his own personal reaction toward the recurrence of the association of *A* with *B,* and as such it is the result of introspection, a piece of psychology, of autobiography. In *The City of Dreadful Night,* James Thomson writes:

"But when a dream night after night is brought
  Throughout a week, and such weeks few or many
  Recur each year for several years, can any
Discern that dream from real life in aught?

"For life is but a dream whose shapes return,
  Some frequently, some seldom, some by night
And some by day, some night and day: we learn,
  The while all change and many vanish quite.
In their recurrence with recurrent changes
A certain seeming order; where this ranges
  We count things real; . . ."

If we overlook the metaphysical distinction between the real and the true, this seems to be sound psychology and there seems to be as much in it as there is in the views of Mr. Russell on induction, and no less. Both may be taken as descriptions of the manner in which we come to be in the possession of a certain opinion, but neither equips us to defend it.[3]

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[1] Russell, *The Problems of Philosophy,* pp. 104–105. See also this JOURNAL, Vol. XIII., pp. 243–245.

[2] Russell, *loc. cit.,* p. 104.

It may be true, as a part of our biography, that we give voice to a "hunch" when we express a judgment of what is probably true, but we need not necessarily have that kind of a biography.

I. THE WILL IN INDUCTION

C. S. Peirce has pointed out that if an array of all the different possible samples, which might be chosen from any collection of any constitution whatever, be made, then (1) there will be a greater number of samples which agree approximately in their constitution with the constitution of the collection from which the samples are taken than there will be of samples which do not agree approximately, and (2) the greater the number of such samples in a collection of samples the closer the approximation of the constitution of the collection of samples to the constitution of the collection from which the samples are taken.

To illustrate, suppose a collection which consists of n objects, O_1, O_2, $O_3 \cdots O_n$, and suppose that two objects are chosen for each sample; how many different possible pairs could be chosen? O_1 may be paired with O_2 and O_3 and so on until it is paired with O_n, yielding $n-1$ pairs. O_2 may be paired with O_3 and O_4 and so on until it is paired with O_n, yielding $n-2$ new pairs, none of which is included in the earlier set. O_x may be paired with each of the objects which is numbered with a number larger than x, yielding $n-x$ pairs, none of which is included in any of the earlier sets. O_{n-1} may be paired with O_n, yielding one pair or $n-(n-1)$ pair. Then the number of the different possible samples of two from a collection of n objects is the sum of the numbers of each of these sets, namely, $n^2-(1+2+3+4+\cdots n)$.

Suppose, now, a collection consisting of n black balls and n red balls. The number of all the different possible samples of two will be $(2n)^2 - (1 + 2 + 3 + \cdots 2n)$. The number of the possible samples of two which are either both black or both red will be $n^2 - (1 + 2 + 3 + \cdots n)$, and the number of both of these, that is, of the different possible pairs which do *not* represent in their constitution the constitution of the original collection, will be $2n^2 - (2 + 4 + 6 + \cdots 2n)$. Then the number of the different possible samples which *do* represent in their constitution the constitution of the original collection will be $[(2n)^2 - (1 + 2 + 3 + \cdots 2n)] - [2n^2 - (2 + 4 + 6 + \cdots 2n)] = 2n^2 - (1 + 3 + 5 + 7 + \cdots 2n - 1)$. This number exceeds the number of non-representative pairs by n. Therefore, if, from a collection consisting of an equal number of black balls and of red balls, all the different possible samples of two be taken, the number of the samples which truly represent in their constitution the constitution of the original collection will exceed the number of non-representative samples by a number equal to the number of balls of either color.

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[3] In this connection a comment of Peirce is interesting. (*Studies in Logic by Members of the Johns Hopkins University*, Boston: Little, Brown and Company, 1883, p. 175.) "The Abbé Gratry believes that the tendency toward truth in induction is due to a miraculous intervention of Almighty God, whereby we are led to make such inductions as happen to be true, and are prevented from making those which are false. Others have supposed that there is a special adaptation of the mind to the universe, so that we are more apt to make true theories [laws?] than we otherwise should be. Now to say that a theory such as these is *necessary* to explaining the validity of induction and hypothesis is to say that these modes of inference are not in themselves valid, but that their conclusions are rendered probable by being probable deductive inferences from a suppressed (and originally unknown) premise."

This same property of collections with reference to samples taken from them may be demonstrated for any collection of any degree of complexity and for samples consisting of any number of objects. To be sure the situation may arise where no sample could possibly represent the collection, as, for instance, if samples of two were taken from a collection containing seventy-seven black balls and thirty-three red balls; yet it is true that the collection of different possible samples will approximate in its constitution, as its size increases, more and more nearly to the constitution of the original collection, and the approximation will reach coincidence if enough samples are taken.[4]

[4] Although the reasoning here is deductive in its nature, I found it expedient in the course of the thinking to perform several simple experiments which served to bring the issues clearly before the mind. In one, a round-bottom, long-neck glass flask of a liter capacity, such as is used in chemical experiments, was taken. Within the flask were a number of balls of cork, some red, some black, of such size that they fell freely through the neck of the flask. The neck of the flask was equipped with a cork stopper, bored, through which a glass plunger operated. This carried, on the end within the flask, a cylinder of cork which acted as a piston and could be adjusted so that, when the flask was inverted, any desired number of the cork balls, up to four, would fall into the neck and could be observed. This device provided a means of securing rapidly a large number of samples. Two red and two black balls were put inside of the flask, and samples of two were taken. After 10 samples had been taken, the collection of all the samples contained 5 reds and 15 blacks. After 25, 20 reds and 30 blacks; after 50, 46 reds and 54 blacks; after 100, 99 reds and 101 blacks. If the apparatus had been constructed so that it positively did not discriminate among all possible samples, then the collection of the samples would represent in its constitution the constitution of the collection of the balls within the flask. After these 100 samples had been taken the approximation was very good. After 150 samples had

We may therefore make the conclusion that, *if samples are chosen from a collection by a method which does not discriminate among the different possible samples, then the collection of the samples so chosen will approximate, as its size increases, more and more closely to the constitution of the collection from which the samples are taken.* This conclusion is demonstrably true, and probability has not yet been introduced.

From these considerations C. S. Peirce concluded that a collection of samples chosen at random *probably and approximately* represents the collection from which the samples are chosen.

If, from the samples we wish to draw inferences relative to the constitution of a collection which is complex in its inward constitution, inferences regarding a considerable number of different characters present or absent in the objects which constitute the collection, as is commonly the case when we make judgments relative to statistical probability, then the degree of approximation is an important *practical* matter. If, as is the case in the inductive inferences of science, we are interested only in a single character or a single relation which is present or absent throughout the collection, then the approximation becomes *is* or *is not* and is readily surpassed if sufficient samples are taken. The question of the degree of approximation, whether in the inductions of science or in the probabilities of the insurance underwriter, disappears if *enough* samples are taken —however many enough may be. It is, in any case, not a logical question; and in the discussion which follows it is assumed that a sufficient number of samples has been chosen and the word "approximately" will not be used. When a judgment of probability is mentioned it is assumed that it is based already on the consideration of so many samples that more samples will not cause any alteration in the judgment. Such a collection of a sufficient number of samples will be referred to constantly hereafter merely as a "collection of samples."

Peirce says: "The instance must be drawn at random. Here is a maxim of conduct."[5] "A random sample is one taken according to a method that would, in the long run, draw any one object as often

been taken, there were in the collection of samples 146 reds and 154 blacks. Of the 150 samples, 23 were black pairs, 19 red pairs and 108 black and red. I can conclude by the reasoning developed in this paper that the hypothesis is probably true that with this apparatus there was a slightly greater tendency for the black balls to fall into the neck of the flask than there was for the red to do so. This hypothesis is verified both by the fact that there were more black pairs than red pairs and by the fact that the representative pairs amounted to more than four sixths of all the samples, for a method of sampling which did not discriminate among the different possible samples would have chosen—in accordance with the theorem above—four representative pairs out of every six.

[5] Peirce, *loc. cit.*, p. 129.

as any other."[6] "Yet I am willing to concede, in order to concede as much as possible, that when a man draws instances at random, all that he knows is that he *tries* to follow a certain precept."[7]

If, from a collection of objects, we make a "collection of samples," a collection, be it noted, sufficiently large to settle absolutely all question of approximation, we still do not know that the collection of samples truly represents the collection from which the samples were taken. We do know that it would represent it if the samples were chosen by some method which does not discriminate among all the possible different samples. We do not know whether our method of choosing fulfils this criterion or not, and we never can know. We know that this criterion can not be fulfilled unless the samples are chosen at random. Moreover, as a sort of clause in the contract, the samples must be so chosen that they are not antecedently likely to conform to the law which is being tested. There may be other criteria which must be followed if we wish to make no discrimination, criteria of which we have no cognizance at present. We know only that if we follow all the criteria of which we know, and choose samples by a method which, *to the best of our knowledge and intent,* makes no discrimination, then the "collection of samples" so chosen *ought* to represent the collection from which the samples were taken.

Probability then depends upon our ignorance of the criteria which will insure perfect impartiality in sampling, and involves our *intent* not to discriminate among possible samples. Probability is only another name for this intent. The principle of induction may be formulated; and, as we now have a definition of probable, the principle of induction has been demonstrated to be true.

THE PRINCIPLE OF INDUCTION.—*A collection of samples, chosen by a method which intends to make no discrimination among all the different possible samples, probably represents in its constitution the constitution of the collection from which the samples are taken.*

If we designate a collection of samples chosen in this way as a "collection of fair samples,"[8] then the principle of induction may be expressed thus: *a collection of fair samples probably represents in its constitution the constitution of the collection from which the samples are taken;* and the expression, a "collection of fair samples," will be used in this sense in the discussion which follows.

The validity of judgments of probability depends upon the will to be fair, impartial, and without discrimination.[9] Without this will we can have no inductive inference and—it is not too much to say it—no scientific method, for most of the methods of reasoning of science may be demonstrated to be consequents of the principle of induction.

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[6] *Ibid.*, p. 157.

[7] *Ibid.*, p. 176.

[8] It will be noticed that "fair sample" here has a slightly different connotation from that which it has in the writings of Peirce and of Royce.

[9] Concerning the scientist's will to be honest methodology has much to say, but there is no place for it here.

II. ANTECEDENT LIKELIHOOD

One of the consequences of our principle of induction is this, that a collection of fair samples justifies us in making an inference *only* as to the probable constitution of that collection from which the samples are *fair.*

An eminent logician, in arguing for the mere enumeration of instances as the basis of inductive inference, has said, in answer to a question, that the observation that each of the swans which are bred from a single pair of swans in a man's back yard is white, is sufficient evidence to permit that man to make the inference that all swans are probably white. Provided that the man has observed "a collection of fair samples" of swans which are bred from a single pair of white swans, and has found them all white, he is justified, as we know, in making the induction that probably all swans which can be bred from the two swans are or will be white. But beyond that he must not go. Moreover, the examination of further swans from the flock in his back yard will not help his case in the least—for these, by our principle, are cases which are now likely to conform to the law which he desires to establish. If he roams about his countryside and looks for swans and observes that every swan that he sees is white, then he may make the induction—though he can not be sure that he has seen all the swans in his neighborhood—that all the swans in his neighborhood are probably white; and so for county, his state, the nation, or the continent. If he wishes to make the induction that all swans on the planet are probably white, he must look for swans in out-of-the-way corners of the world, in those places where it is not antecedently likely that swans will be white. Unless he does so his generalization, which applies only to that collection of swans among all the possible different samples of which he intended not to discriminate, will be valid only for a part of all the swans. Let him examine especially the swans of Australia, where there are black swans, or the swans in Los Angeles.

To establish a law which is probably *valid for all,* samples must be chosen which are not antecedently likely to conform to the law which has already been established for some special collection which is a part of the all.

III. THE METHOD OF VERIFICATION IN SCIENCE

In addition to his contributions to the *logic* of induction, Peirce also had something to say about certain psychological considerations, traits of mind, which it involves. His views may perhaps be indicated fairly yet briefly by the following formulation.

Observation: A is B.

Retroduction: Probably all A's are B's.

Verification: A collection of fair samples of A's is made, and each A is found to be a B.

Induction: Probably all A's are B's.

The interesting thing is the retroduction (called also by Peirce the "abduction"); yet in the case where the probable constitution of a collection is inferred from the known constitution of a collection of fair samples, there is no necessity for it. Any logical mind, a mind let it be supposed which never makes a guess and never has an intuitive glimpse or flash of suspected truth, but devotes itself wholly to the elaboration of necessary consequences, could perfectly well by our principle derive the *induction* from the *verification* alone, and, in so far as it is merely logical, could not fail to do so. But retroduction is exceedingly important when considered in connection with the so-called Baconian method or the method of working hypotheses.

Observation: F (fact) is observed.

Retroduction: H (hypothesis) explains F.

Verification: H explains also F_1, F_2, F_3, *etc.* (other and different fair sample facts), which are sought out and observed.

Induction: H is probably true.

The fact that the working hypothesis is probably true will be demonstrated later to be a consequence of the principle of induction, and it need cause us no further concern at the moment. But the matters which depend upon the primary choice of the hypothesis are also important in their bearing on the method of science.

The number of hypotheses which might serve to explain a given fact is well-nigh infinite: the number of those which are actually considered and tested by the scientist is very small. As an historical fact the scientist exhibits an almost uncanny facility in selecting the right hypothesis from among the infinite of otherwise equally plausible hypotheses. Kohlrausch said of Faraday, "Er riecht die Wahrheit." Ernst Mach says,[10] "Every experimenter can daily observe in himself the guidance that instinctive knowledge furnishes him. If he succeed in abstractly formulating what is contained in it, he will as a rule have made an important advance in science. . . . Indeed, it is perfectly certain that the union of the strongest instinct with the greatest power of abstract formulation alone [?] constitutes the great natural inquirer." Charles Peirce writes,[11]

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[10] Ernst Mach, *The Science of Mechanics* (tr. by Thomas J. McCormack), p. 27.

[11] C. S. Peirce, "A Neglected Argument for the Reality of God," *The Hibbert Journal,* October, 1908. Quoted passage, p. 106. Also Peirce, *Studies in*

"Modern science has been builded after the model of Galileo, who founded it on *il lume naturale.* That truly inspired prophet has said that, of two hypotheses, the *simpler* is to be preferred; but I was formerly one of those who, in our dull self-conceit fancying ourselves more sly than he, twisted the maxim to mean the *logically* simpler, the one that adds the least to what has been observed, in spite of three obvious objections: first, that so there was no support for the hypothesis; secondly, that by the same token we ought to content ourselves with simply formulating the special observations actually made; and thirdly, that every advance in science that further opens the truth to our view discloses a world of unexpected complications. It was not until long experience forced me to realize that subsequent discoveries were every time showing I had been wrong, while those who understood the maxim as Galileo had done, early unlocked the secret, that the scales fell from my eyes and my mind awoke to the broad and flaming daylight that it is the simpler hypothesis in the sense of the more facile and natural, the one that instinct suggests, that must be preferred; for the reason that unless man have a natural bent in accordance with nature's, he has no chance of understanding nature at all."

The source of the scientist's intuition is doubtless to be found in his wide experience of the phenomena with which he deals. He has a facility, resulting from this intuition, in selecting the right hypothesis. Yet hypotheses alone are not the only result of scientific activity, and intuition does not "alone constitute the great natural inquirer." Science has its theories—which are statements of the *conditions without which* certain facts could not be. Theories are implied by facts, and for their production only the coldest logical sense is needed. The very "spontaneous conjectures of instinctive reason" which have helped the scientist in his search for hypotheses have also hindered him in his search for theories. He has frequently been led to believe that the facts imply things which are really not implied by them. He has accepted false theories and has been slow to accept new and true ones.[12]

Logic, p. 108, "Nature is a far vaster and less clearly arranged repertory of facts than the census report; and if men had not come to it with special aptitudes for guessing right, it may well be doubted whether in the ten or twenty thousand years that they may have existed their greatest mind would have attained the amount of knowedge which is actually possessed by the lowest idiot" (p. 181). "Side by side, then, with the well-established proposition that all knowledge is based on experience, and that science is only advanced by the experimental verification of theories [hypotheses?], we have to place this other equally important truth, that all human knowledge, up to the highest flights of science, is but the development of our inborn animal instincts." I say, not *all!*

[12] For a fuller discussion of this point, with instances from the history of science, see "Intuition in Science," *University of California Chronicle,* April, 1917.

IV. THE SUPPOSED ASSUMPTION IN SCIENCE OF THE UNIFORMITY OF NATURE

In his *Introduction to Science* J. Arthur Thomson says (p. 77): "There is one fundamental postulate underlying scientific procedure—a postulate which is verified with every fresh step. It is the postulate of the Uniformity of Nature. This, which may be analyzed into a number of postulates, means that for our human purposes there is stability in the properties of things, that the same situations are continually recurring, that there is a routine in the order of nature—a routine without gaps or interpolations, in which every event is determined by antecedent events."

Peirce says on the contrary:[13] "That there is a general tendency toward uniformity in nature is not merely an unfounded, it is an absolutely absurd, idea in any other sense than that man is adapted to his surroundings. For the universe of marks is only limited by the limitation of human interests and powers of observation. Except for that limitation, every lot of objects in the universe would have . . . some character in common and peculiar to it. Consequently, there is but one possible arrangement of characters among objects as they exist, and there is no room for a greater or less degree of uniformity in nature. If nature seems highly uniform to us, it is only because our powers are adapted to our desires."

Whether men of science believe in the uniformity of nature, it seems hardly worth while here to inquire, for many of them have confessed it, and many writers on the method of science have believed it. We measure the specific gravity and melting point and other "constants" of one pure sample of some substance and blithely suppose that our measurements are correct (within the limit of error) for all pure samples of the same substance. Yet a knowledge of the status of such a belief is important for an inquiry into the basis of inductive inference, and consequently important for an understanding of scientific method. Is it merely a "postulate," an assumption, a premise granted in order that the scientist may make progress in his reasoning? or is it perhaps a popular error, a belief that is really not necessary for science? or is it supported by evidence, a belief derived from some earlier assumption or experience?

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[13] Peirce, *Studies in Logic*, p. 178. Doubtless one reason for Peirce's animosity toward the belief in the uniformity of nature is to be found in his belief that the laws of nature are themselves undergoing evolution—a belief based upon his exact gravitational measurements, and consequently developed in his mind first with reference to the law of gravitation. We now know that the law of gravitation as formulated by Newton *never was right*. R. C. Tolman has shown (*Physical Review*, N. S., III., 244, "The Principle of Similitude") that the attractive force between two masses can not possibly be directly as the product of the masses and inversely as the square of the distance between them in any world where (as seems to be the case in this one) the velocity of light is absolute and the charge of the electron is absolute.

That there is a seeming constancy in the observed properties and relations of things, that there is uniformity in "the universe of marks," no observant person will deny. The fact that the data of experience appear time after time in the same order and with the same relations among themselves is itself a fact of experience. If, now, we are permitted to make an inductive generalization, we may judge that the various kinds of experience which have occurred in certain orders *probably* always occur in those orders. Since experience is the *Ausgangsmaterial* of all science, this is as much information about the uniformity of nature as science will ever need. This is the *law* of the unformity of nature. As such it is a true statement of probability. It is not an assumption.

To point out, as Peirce has done, that the universe of marks is limited by human interests and powers of observation, does not in any way affect the validity of the law of the uniformity of nature. The law, of course, applies only to palpable nature; but that is all that the laws of science can claim to deal with, anyhow. Science uses hypotheses and theories in its treatment of inaccessible nature and of impalpable attributes.[14] Moreover, if scientific method is to have a sound basis in epistemology, then its laws must necessarily refer only to the kinds of things that are given in experience.

But inductive inference itself involves one assumption, and without it there can be no law of the uniformity of nature, and no other law. The principle of induction is based upon our knowledge of the relation between the constitution of a collection of samples and the constitution of the collection from which the samples are taken. If, then, we collect the data of experience and proceed to generalize upon them, we have tacitly made the assumption that *there is a collection of real objects which are in their marks what they are absolutely* (and all our piety and wit can neither make nor mar them). The assumption may be expressed in a manner which possibly savors less of metaphysical connotations: *there exists a real collection of potential experiences, each member of which is what it is absolutely.*[15] This assumption is involved also in the other, and as I believe, inadequate, formulations of the principle of induction—Russell's, for instance. If one has a "hunch" that A and B are probably always associated, he presupposes the collection (which is not yet given in experience) in which the association to be given will occur. In this paper I shall not take it upon myself either to defend this assumption or to examine its metaphysical consequences. I only point out that it is the fundamental postulate of all inductive thinking and of much of natural science.

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[14] The knowledge of the inaccessible qualities of nature, as, for instance, of the atomic constitution of matter, involves some delicate metaphysical considerations. One may say at least that the experience of isomeric substances gives also, in whatever sense that it gives the substances themselves, *implicitly* the information that the substances are atomic in structure. The judgment that "that" (object of experience) is red has no more warrant than the judgment that "these" (objects of experience, isomers) are atomic. Thus one is led to the question of the nature of the objects which are themselves the subjects of judgments which are warranted by experience and of the application of such judgments in general.

[15] The interesting inquiry into the exact scope of the *will* in a universe constructed on this plan is not undertaken here.

V. THE CALCULUS OF FAIR SAMPLES

The calculus of fair samples is devoted to the development of the consequences of the principle of induction. Between it and the algebra of logic our manner of knowing all things except the metalogical and the other, ordinary, information of experience is comprehended.

Let a bold-faced capital **S** be used to denote a collection of fair samples (as defined above); let it be followed by the name in parenthesis of the character or characters possessed by the samples. Let the symbol π be used to denote probability.[16] If the universe of discourse is understood to be the collection from which the samples are *fairly chosen,* then the principle of induction may be symbolized.

$$(1) \qquad \mathbf{S}(a) = \pi(a);$$

which means that a collection of fair samples which present the property a is the same as the probability that a is present in every member of the collection from which the samples are chosen. Likewise we have, where R is any relation—

$$(2) \qquad \mathbf{S}(aRb) = \pi(aRb).$$

Thus it appears that the "association of A with B" which was used by Mr. Russell in his statement of the principle of induction is only a special case of the more general aRb. R may denote the relation of logical addition or of logical multiplication, giving us $\mathbf{S}(a+b) = \pi(a+b)$, a collection of samples in each of which either a or b is present is the same as the probability that either a or b is present in every member of the collection from which the samples are taken, and $\mathbf{S}(a, b) = \pi(a, b)$, a collection of samples in each of which both a and b are present is the same as the probability that both a and b are present in every member of the collection from which the samples are taken. The case in which R is the implication relation is especially important, for it leads to the proof of the validity of the method of verification. Let H stand for an hypothesis, then

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[16] Other symbols are as in the algebra of logic; $-\!\!<$ for implies, 1 for true, 0 for false, *etc.* No effort is here made to use symbols in the same manner in which they are used in the symbolic logic of classes. The intelligible but discursive statement which I have called the principle of induction seems to me to be equally intelligible if expressed $\mathbf{S}(a) = \pi(a)$, and the effort here is merely to express certain of its intelligible consequences in intelligible symbols for the sake of the logical connections that seem to be thus rendered more obvious. To express these relations in terms of the conventional algebra of classes I already know is not easy: it will be a task for the future.

(3) $$\mathbf{S}(H -\!\!< 1) = \pi(H -\!\!< 1),$$

But $$(H -\!\!< 1) = (H = 1) = (H).$$[17]

Hence $$\mathbf{S}(H -\!\!< 1) = \pi(H).$$

An hypothesis which implies the true (and that which is experienced is taken in science as true) in a collection of fair sample cases is the same as the probability that the hypothesis always implies the true in the universe of discourse, is the same as the probability that the hypothesis is true.

When a particular fact implied by an hypothesis has been given in experience, we have at hand one case in which the hypothesis has implied the true. By the general law of the uniformity of nature it is antecedently likely that other facts like the first, and equally implied by the hypothesis, may be found; but these will now be without value for establishing the probability of the hypothesis. Other facts of wholly different kinds must be sought out, of such kinds that each kind is implied by the hypothesis. Thus, if we hold the hypothesis that the barometer column is supported by the weight of the atmosphere and see, moreover, that our hypothesis implies that the height of the barometer column would be less at the top of a mountain than at sea-level, then, if we climb a mountain with our barometer and observe that its height is less than it was when we left the foot of the mountain, we have found one case in which our hypothesis implies the true. If we carry our barometer to the summits of other mountains, because of the very nature of the principle of induction, we have not improved the status of our hypothesis in the least. We must seek out other kinds of situations; we may carry our barometer below sea-level into a deep mine, or we may put it under an air pump. The numerous verifications of an hypothesis are determined by the ability of the hypothesis to explain *different* situations.

Since the ordinary algebra of logic applies to collections and since collections are the subject-matter of the calculus of fair samples, we have—

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[17] In the ordinary algebra of logic there is nothing which corresponds to $(H -\!\!< 1) = (H = 1) = (H)$. This piece of symbolism may be regarded as shorthand for the logical truism that a proposition which invariably implies the true is itself true. See this JOURNAL, Vol. XIV., p. 96.

(4) If $a -\!\!< b$, then $\mathbf{S}(a) -\!\!< \mathbf{S}(b)$, and $\pi(a) -\!\!< \pi(b)$.

(5) $\mathbf{S}(a), \mathbf{S}(b) = \mathbf{S}(a, b) = \pi(a, b)$; and $\pi(a), \pi(b) = \pi(a, b)$.

(6) $\mathbf{S}(a) + \mathbf{S}(b) = \mathbf{S}(a + b) = \pi(a + b)$; and
$\pi(a) + \pi(b) = \pi(a + b)$.

From the principle of induction we have immediately the theorems—

(7) $\mathbf{S}[\pi(a)] = \pi[\pi(a)] = \pi^2(a)$.

(8) $\mathbf{S}[\pi^2(a)] = \pi[\pi^2(a)] = \pi^3(a)$, *etc.*

And it appears that we may speak with justice of the *powers of probability,* for the status of the probably probable is different from that of the merely probable. For, if by the examination of fair samples from a defined collection we establish the truth that probably every member of the defined collection is *a,* and if then we proceed to examine as samples a number of such defined collections all of which are within some larger defined collection and find that for each of these smaller collections, which as samples we have examined, it is probable that every member is *a;* then, though we may not conclude that every individual member of each of the smaller collections and hence of the larger collection is probably *a*—for we have not examined *every one* of the smaller collections, still we may conclude that it is *probably probable* that every member of each of the smaller collections and hence of the large collection is *a.* And so for any power of probability.

The greater part of the working hypotheses of science is *probably probable*—for in most cases the validity of the implication of the facts by the hypothesis is itself dependent upon some law of nature which is probably true. The situation may be symbolized.

(9) If aRb, then $H -\!\!< 1$; but $\mathbf{S}(aRb) = \pi(aRb)$,
and $\pi(aRb) -\!\!< \pi(H -\!\!< 1)$;

Hence

$$\mathbf{S}[\pi(H -\!\!< 1)] = \pi[\pi(H -\!\!< 1)] = \pi^2(H -\!\!< 1).$$

A collection of fair sample cases in each of which the hypothesis probably implies the true is the same as the probable probability of the truth of the hypothesis.

The hypothesis that salts in aqueous solution dissociate into positively charged metal ions and negatively charged acid ions implies the phenomena of electrolysis, if the law of electrostatics (which of course is probable) that unlike charges attract one another, be taken as true, it implies the abnormally large boiling-point raising and freezing-point lowering and osmotic pressure which are shown by salt solutions, if the laws (again probable) which connect these things with concentration be taken as true, and so on. The hypothesis of electrolytic dissociation is probably probable.

A theory in science is an opinion which is somehow implied by the facts; and theory may be defined, $F -\!\!< T$, fact implies theory. The phenomenon of interference implies that the particular light of which the phenomenon is observed is a kind of wave motion. This theory, as related to the particular instance, is true. If now I examine a collection of fair samples of light and find interference in each case, I may conclude that it is probable that all light is wave motion. This is a case of Theorem 4 above. In many cases, however, the implication of the theory by the fact is only probable and depends for its validity upon the validity of some law. Thus

(10) If aRb, then $F -\!\!< T$; but

$$\mathbf{S}(aRb) = \pi(aRb), \quad \text{and} \quad \pi(aRb) -\!\!< \pi(F -\!\!< T);$$

Hence

$$\mathbf{S}(aRb) -\!\!< \pi(F -\!\!< T) -\!\!< \pi T,$$

where F is a fact, *i. e.*, a true particular. From the principle of the syllogism, namely, $(a -\!\!< b)(b -\!\!< c) -\!\!< (a -\!\!< c)$, we have other theorems.

(11) If $(a -\!\!< b)(b -\!\!< c)$, then $\pi(a) -\!\!< \pi(c)$.

(12) If $\pi(a -\!\!< b)(b -\!\!< c)$, then $\pi(a) -\!\!< \pi^2(c)$.

(13) If $(a -\!\!< b)$, $\pi(b -\!\!< c)$, then $\pi(a) -\!\!< \pi^2(c)$.

(14) If $\pi(a -\!\!< b)$, $\pi(b -\!\!< c)$, then $\pi^2(a -\!\!< c)$,
or $a -\!\!< \pi^2(c)$, or $\pi(a) -\!\!< \pi^3(c)$.

These theorems and others are used in the production of the theories of science; and there are theories which are true, probable, probably probable, and so on.

The fourteenth theorem is illustrated by reasoning which has been used by the climatologist, Ellsworth Huntington,[18] in connection with the *Sequoia washingtoniana* of the Sierra Nevadas. If it is probable that the number and sequence of the annual rings indicate the number and sequence of the years of age of the tree, and if it is probable that a sequence of unusually narrow annual rings indicates a sequence of dry years, then it is probably probable that a sequence of narrow annual rings corresponding to the period about 870 years before Christ indicates that there was a drought in California at about the time that there was famine and drought in Palestine in the days of Elijah.

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[18] Huntington, *The Secret of the Big Trees.* Publication of the U. S. Department of the Interior. "In regions having a strongly marked difference be-

The probably true theory of science is distinctly a different kind of belief from the probably true hypothesis. The hypothesis is only one of the many possible explanations of the facts: the theory *in the light of all of the knowledge of science* is the only possible one; and if the laws of nature are true, then the theory also is necessarily true. Though some theories are probable, though no hypotheses are true, yet the faculties of mind requisite for the production of the two kinds of belief and the motives behind their use are widely different.

The foregoing are but a few of the theorems of the calculus of fair samples, and more will be developed in the future. The calculus gives us the means of defining exactly the various kinds of scientific belief, laws, hypotheses, theories. It equips us with the apparatus for that "stock-taking" in science for which Huxley pleaded so eloquently.

VI. STATISTICAL PROBABILITY AND DEGREE OF PROBABILITY

Probability is the name for our intent to choose samples fairly. Our intent is an effort to do the best that we can under the limitations of our ignorance. We never can be sure that we have examined every member of the collection about whose constitution we wish to make an inference. We never can be sure that the samples which we have examined have been chosen by a method which does not discriminate among possible samples. We know, however, that we might conceivably choose so many samples that the further examination of samples will not cause us to alter our inference. This point marks the limit of available knowledge. Yet at this point our knowledge is only knowledge of the *probable*—for we are not, and never can be, "cock-sure" of our samples. *Probability, therefore, does not approach certainty without limit.* An inference may conceivably be one hundred per cent. probable. If it were, it would still not be true.

tween summer and winter it is well known that trees habitually lay on a ring of wood each year. . . . And it is now established that in regions with cold winters and dry summers the thickness of the annual layers of growth gives an approximate measure of the amount of rain and snow" (pp. 10–11).

It seems reasonable to suppose that one hundred per cent. probability is a thing that is attainable only in ideal. However, the fact that it would, if attained, still not be certainty, involves some interesting consequences—especially in the use of words. A high probability is a probability which will be altered but slightly by the further examination of samples. A low probability will be altered more widely; but even the low probability has been founded on the examination of *some* samples and has already begun to approximate to the highest probability which may be attained. Our inductive knowledge is not to be tabulated on the scale of impossibility—improbability—probability—certainty, as some logicians have held; but rather—if the present formulation of the principle of induction is correct—on the scale of ignorance—knowledge of probability—knowledge of the highest attainable probability. We never have certainty of the constitution of a large collection in the sense that we have certainty of the nature of the particular sample.

Suppose that I am given a box containing balls, black or red; and suppose moreover that I agree not to open the box. There is a glass window in its side through which I may see one ball and only one. I am asked to make an estimate of the constitution of the collection of balls. If now I shake the box and look through the window and see a black ball, I am justified in making the judgment that there is a slight probability that all the balls in the box are black. The probability is slight because it is based on meager evidence. But when I say that the probability is slight that all the balls are black, I do *not* mean that it is highly *improbable* that all the balls are black—a meaning which is sometimes attached to the expression. The probability is slight, but it is none the less genuine. This use of the words is the consequence of the nature of our warrant for making probable inferences. If now I shake the box many times and examine many samples, and if I find that each sample is a black ball, then I may justly make the inference that it is highly probable that all the balls in the box are black. On the other hand, if I find that seven tenths of the many samples are black, then I may infer that it is highly probable that seven tenths of all the balls in the box are black. I have absolutely no warrant for so perverting the language of my fathers as to say that it is seven tenths probable that the next ball I examine will be black. I could have said that (if at all) with equal propriety after the examination of the first ten samples. At that time my warrant for the inference was small and the probability was small; now my warrant is large and the probability is large. At that time it would have been correct for me to infer that the probability is small (but genuine) that seven tenths of the balls in the box are black: now I may infer of the same thing that the probability is large. I might, by continuing to examine samples, conceivably reach the point at which further samples would be without effect on the nature of my inference. But if I should by any chance reach this point I would not know it. All I could infer would be that the probability is great. It follows, therefore, from our principle of induction that the degree of probability is not a thing which submits to being expressed exactly in numbers—we may speak of low probability or high, but we must not speak of probability as a ratio.

We must not say that the probability is fifty-fifty that a certain collection has a specified constitution: we must say that the probability is that a certain collection has a fifty-fifty specified constitution. Accordingly, if $F(a)$ denotes the fraction of all the fair samples which present the property a, we have for statistical probability the expression

$$(a) \qquad \mathbf{S}F(a) = \pi F(a).$$

If a fraction F of the objects in a collection of fair samples possesses the property a, then it is probable that a is possessed by the fraction F of the objects in the total collection from which the samples were chosen.

There will be similar expressions for the logical products and the logical sums of different properties probably present in corresponding fractions of the *same* collection.

$$(b) \qquad \mathbf{S}F_1(a) \cdot \mathbf{S}F_2(b) = \mathbf{S}F_1F_2(ab) = F_1F_2(ab).$$

If it is probable both that half of all the musicians use tobacco and that one quarter of all the musicians wear whiskers, then it is probable that $\frac{1}{2} \times \frac{1}{4} = \frac{1}{8}$ of all the musicians both use tobacco and wear whiskers.[19]

$$(c) \qquad \mathbf{S}F_1(a) + \mathbf{S}F_2(b) = \mathbf{S}[F_1(a) + F_2(b)] = \pi[F_1(a) + F_2(b)].$$

[19] It might be objected that this theorem is not necessarily true for the reason that the half of the musicians who use tobacco might possibly be an isolated group none of whom wears whiskers. Such a state of affairs, because of the very nature of the fair sampling process, would lead us, if it existed, to the inference that it is probable that a certain half of all the musicians, namely, that half which is found among those who do not wear whiskers, all use tobacco. It would be antecedently more likely that a musician who wears whiskers does not use tobacco than that he does. Only if there are no observations which make any correlation of whiskers and the use of tobacco antecedently likely is it possible to have two genuine inductive inferences, regarding the use of tobacco and whiskers among musicians, both of which have been founded on the examination of cases which are in no way antecedently likely to conform. If the inferences are valid, they have been founded on the examination of fair samples, and the musicians who are tobacco-users are distributed evenly, to the best of our knowledge and intent, among the collection of all the musicians, and the theorem is correct as stated.

If $$F_1 = f_1/D \quad \text{and} \quad F_2 = f_2/D,$$

then $$\mathbf{S}F_1(a) + \mathbf{S}F_2(b) = (\pi/D)\,[f_1(a) + f_2(b)].$$

If it is probable either that half of all the musicians use tobacco or that one quarter of all the musicians wear whiskers, then it is probable that of every four musicians either two use tobacco or one wears whiskers.

It would be desirable to be able to treat by the method of this calculus the logical sum and the logical product of different characters probably present in the corresponding fractions of *different* collections. Thus, if it is probable that half of the musicians use tobacco and if it is probable that a third of the poets carry walking-sticks, it would be interesting to know what probable inferences can be made (1) about those who are *both* poets and musicians, (2) about those who are *either* poets *or* musicians *or both,* and (3) about those who are *either* poets *or* musicians, *but not both.* Limitations of space prevent that this matter shall be developed here; moreover the requisite symbolism is intricate, and it may reasonably enough be questioned if symbolism adds anything here to the clarity of the thought. There are, however, two points that are important for the present consideration.

If we definitely knew that half of the musicians were tobacco-users and that a third of the poets carried walking-sticks and that these were distributed evenly among their respective collections, we could conclude that in the collection of those who are both musicians and poets one half use tobacco and one third carry walking-sticks, but we could still not draw valid inferences about the logical sums unless we knew in addition the relative sizes of the collection of all the musicians and of all the poets and the extent to which these collections overlap. Thus, if we knew that there were five times as many musicians as there were poets and that a seventh of all the poets were musicians (and consequently that one thirty-fifth of all the musicians were poets), we could conclude (1) that in the collection of those who are *either* musicians *or* poets *or both* five twelfths are tobacco-users and one eighteenth carry walking-sticks, and (2) that in the collection of those who are *either* musicians *or* poets, *but not both,* seventeen fortieths are tobacco-users and one twentieth carry walking-sticks.

If our information about the fraction of all the musicians who use tobacco and about the fraction of all the poets who carry walking-sticks is itself the result of an inductive inference—as is commonly the case—then our conclusions about those who are both musicians and poets are *probably true.* The further information which is necessary in order that we can make inferences about the logical sums is ordinarily information that has been collected by statistical means, and is probable. Our inferences, therefore, about the logical sums of *different* collections are ordinarily probably probable.

The statistical probability which is the consequence of the principle of induction should, in the future, be correlated and compared with the various other theories of probability. In particular its connection with the "probability" which involves the *calculation of chances* should be determined.

The principle of induction is a consequence of certain properties of collections. As a consequence of these properties there are also certain rules which must be followed in applying the principle. As another, it appears that the only meaning which can be attached intelligently to the word *probable* is this—that the probable is that which would be true if we knew that our will to choose fairly among all possible samples was successful. The principle of induction derives its validity from the metaphysical assumption that there exists a collection of potential experiences each member of which is what it is absolutely. From this assumption the propriety of many of the methods of reasoning of the natural sciences may be demonstrated.

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A CLUE TO HOLT'S TREATMENT OF THE FREUDIAN WISH

I FIND myself in close agreement with Professor Watson's recent estimate[1] of Edwin Holt's brilliant little book on *The Freudian Wish*. "That Holt's wish is not Freud's" (p. 86²) and that there is —from my point of view, very fortunately—more of Holt than of Freud in the book (p. 90³); that we none the less are indebted to Holt for his emphasis on Freud's genuine contribution to general psychology; that Holt is justified in his effort to replace the sensation as unit of psychology (p. 86¹); that his effort is not wholly successful and that, in particular, he does not present us with a satisfactory substitute for "Meynert's justly criticized scheme"—to all these conclusions I subscribe very cordially. I also agree with Mr. Watson that the book has a thoroughly "behavioristic tendency" (p. 90⁴), while yet "in many places" it has not rid itself from "subjectivism." But Watson, I think, has not found the clue to Holt's apparent seesaw between "behaviorism" and "subjectivism." Yet it lies ready

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[1] This JOURNAL, Vol. XIV., pp. 85–92.